logical maps. The map of the British Isles was handed in for publication at the closing meeting. Very little time was given to the map in the public sessions of the Congress, but the Map Commission had three long sittings, the results of which will be printed in the official Report. The most important points arrived at were the adoption of the term *Pleistocene* for the index of the map (the German term "*quartär*" to be bracketed with this); the separation of the modern deposits from the Pleistocene, and the mapping of the latter wherever practicable, the underlying formations (where known) to be distinguished by coloured lines; in modern eruptive rocks (those of volcanoes now active or only recently extinct) the stratified volcanic tuffs are to be distinguished from the cinders and the scoriæ.

M. Karpinski has been the representative of Russia on the Map Commission. On this occasion he was not present, his place being taken by MM. Nikitin and Tschernicheff. The latter submitted an important note on the crystalline schists of the Ural Mountains, which would have enlivened the discussion upon this question in the public meetings of the Congress. He states that the crystalline schists of the Urals contain limestones with a distinct hercynian fauna, and also that the schists pass horizontally into Devonian strata. It is probable that in cases of this kind (and similar cases elsewhere were referred to in the public discussion) the schists will be represented by the colour denoting their presumed age, whilst their present lithological character will be denoted by coloured lines. M. Nikitin raised a point which is important in many parts of Europe, but which is especially so in Russia—that is, the necessity of distinguishing *transition-beds*. He instanced the Volgian beds, which link the Jurassic with the Cretaceous; the Tartarian, between the Permian and the Trias; and others, spoken of by M. Nikitin as Permo-Carboniferous, which link the Permian to the Carboniferous. These transition-beds occupy immense areas in Russia, and cannot well be fitted into the existing classification.

The discussion on the crystalline schists occupied the whole of the sitting on Wednesday, and part of that on Friday. The material for this discussion had been provided by a collection of papers printed in advance and distributed at the opening. Translations from parts of this polyglot pamphlet have now appeared in NATURE. Essays in English were also contributed by five officers of the United States Geological Survey, with an introduction by Major Powell; and by Mr. Lawson, of the Geological Survey of Canada. One by Reusch, on Norway, also in English, was received too late for printing in the pamphlet, but it will appear in the full Report of the Congress.

This discussion derived additional value from the fine collection of rocks, maps, lectures, &c., illustrating this particular subject close at hand in the temporary Museum. The Geological Survey exhibited a large collection of rocks, maps, sections, &c., illustrating the North-West, the Central, and the Southern Highlands of Scotland; important collections of British rocks were also exhibited by Bonney, Blake, Hicks, Callaway, Cole, Hatch, Rutley, Wunsch, and others; foreign rocks were exhibited by Bell from Canada, Delgado from Portugal, Torell from Sweden, Reusch from Norway, Giordano and Mattirolo from Italy; whilst maps, drawings, models, &c., illustrating the discussion, were exhibited by Teall, Baltzer, Cadell, Ricketts, Lapworth, and others. Special mention should be made of the splendid collection exhibited by Heim, illustrating the deformation, crushing, &c., which the rocks of the Alps have undergone. All these exhibits are described in the Catalogue (54 pages with supplement of 4 pages). Several members of the Congress assisted in the arrangement of this Museum, but its success was chiefly due to the labours of Dr. Hinde, Mr. Teall, and Mr. Rudler.

In the foregoing notes we have not attempted to summarize the discussions. These were reported at some length in the *Times* and in other papers. We have preferred to devote the space at our disposal to a general survey of the meeting, and to note some points of importance which could not well be included in a formal report of daily proceedings. As already stated, the discussions may by some be held to have led to no definite result, inasmuch as no vote was taken and therefore no formal decision of the Congress can in future be appealed to. But the great value of such meetings lies in the opportunity afforded for personal discussion and the interchange of opinions, not only in the public sessions, but in the more easy and informal conversations over the exhibits in the Museum, in the corridors and reading-room, and at the friendly and social gatherings which made so pleasant a feature of the London meeting. We have no doubt that the general result of this meeting on geological opinion and progress will be at least as good as that of any which has gone before.

The London Congress was particularly fortunate in its place of meeting. Within the walls of the University of London there was ample accommodation for all the requirements of the Congress, whilst close at hand were the Jermyn Street Museum and the rooms of the Geological Society. Unfortunately the Honorary President, Prof. Huxley, was kept away by ill-health; Prof. Hughes, who has done so much for the Congress in England, was also unable to attend. The early death of M. Fontannes, who has so ably reported the proceedings of previous meetings, is a great loss to the Congress, and many fears were expressed that his place could not be adequately filled; but the labours of Messrs. Hulke and Foster in the Council, and of Barrois and Renard at the meetings, resulted in fuller reports than have appeared of any previous Congress.

REMARKS ON SOME OF THE MORE RECENT PUBLICATIONS DEALING WITH THE CRYSTALLINE SCHISTS.[1]

IN acceding to the invitation of the Geological Congress to contribute to the discussion of the crystalline schists, the author expresses his regret that his time has not allowed him to throw new light by fresh observation on the points of controversy. Other labours have for a long time completely occupied him; so that he has only been able to occasionally assist with advice a younger fellow-worker, Herr Emil Danzig, of Rochlitz, in his researches on the Saxon granulites. This work, which has but recently been brought to a close, and has been placed at the disposition of the members of the Congress, is recommended to the notice of those fellow-workers who are interested in these matters, for in it the granulite question has been completely treated and advanced another stage.

Prof. Lehmann still takes his stand on the results furnished him four years ago by his investigations on the old crystalline schists.

The, on the whole, favourable reception of those investigations assuredly indicates that the right path has been struck, and that an extension of our views on the crystalline schists has resulted from them. This is also proved by the fact that these views have also been successfully applied in other places. That in many cases the opinions advocated by the author have not been rendered quite correctly, cannot excite surprise. Such misconceptions were scarcely to be avoided.

Prof. Lehmann strenuously opposes the notion that his generalizations were made without due consideration, and draws attention to certain criticisms to which his work has been recently subjected.

As is well known, the controversy on the Saxon granulites turns on the question, whether their plainly developed parallel structure is to be regarded as true bedding in the sense of sedimentary deposition, or as of eruptive or plutonic origin. The same questions arise in the discussion of all other districts in which crystalline schists occur; the solution, however, will by no means always be the same. It is beyond doubt that a whole series of crystalline schists are of sedimentary origin, and it is a matter to be decided by detailed investigation which are to be considered as sedimentary and which as eruptive or plutonic. The results obtained by the author in the investigation of the Saxon "Granulitgebirge" and some adjacent districts do not therefore claim universal application.

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[1] "Bemerkungen zu einigen neueren Arbeiten über Krystallinisch-schiefrige Gesteine," by Prof. J. Lehmann. Published by the International Geological Congress, London, 1888. (Abstracted from the German by Dr. F. H. Hatch.)

The tentative interpretations given by him were arrived at by the close observation of the field-relations of the rocks in question during a geological survey extending over several years; and it can now only be a question in how far the interpretation, which has been recognized with certainty as correct for a series of phenomena, can be applied to other phenomena intimately related to them. The author admits that here and there he has gone somewhat too far in his tentative interpretation. It was scarcely possible, in so difficult a question as the "granulite question," which to-day has not yet reached its final limits, to go just so far that later experience should find nothing to modify.

But the description of the author's work by J. Roth (in a paper on "Zobtenite," read before the Berlin Akademie der Wissenschaften on June 23 of last year) as "a marvellous agglomeration of the most daring hypotheses" is scarcely justifiable.

In Prof. Lehmann's investigations on the crystalline schists it has, for the first time, been shown in the greatest detail that their present condition cannot be original, but must be one that has been influenced by the dynamic processes accompanying mountain-building. He is far from maintaining, however, that similar observations had not already been made; and he readily acknowledges that eminent investigators of the crystalline schists, such as Kjerulf and Michel-Lévy, had, at a much earlier period, made such observations. What is new is the mode and method in which the author utilizes his observations. Researches of this kind were sunk into oblivion: the theory of the sedimentary origin of the crystalline schists had become the ruling dogma; and the *Eozoon canadense* had also made its appearance in Europe.

Roth, in the paper referred to, maintains his old position, according to which the crystalline schists, including the phyllites, are plutonic and unaltered formations.

The evidence advanced by him to prove that the stratiform gabbros, which he terms zobtenite, cannot be numbered with the eruptive rocks is insufficient. The occasional observation of conformable relations with other crystalline schists is inadequate. This does not, however, hinder Roth from regarding it as proved that the Zobten rock cannot be eruptive. The isolated patches of the old rocks that crop out in Silesia are unfortunately extremely confused. The stratigraphical relations of these rocks, which are very highly metamorphosed, cannot be utilized to support either view, and no hope is to be entertained of more favourable exposures in the future.

Prof. Lehmann's views on the Saxon granulites have, in the main, been confirmed by the before-mentioned work of Herr E. Danzig. This work again shows how confused are the field-relations in the granulite-district, and that few exposures permit of an indisputable solution.

In the northern half of the Saxon district the granulite assumes a granular structure, and acquires a marked similarity to some "bedded" granites. These points have received especial attention from Herr E. Danzig. He comes to the conclusion that in many places no sharp line can be drawn between granulite and granite; further, that rocks, which belong undoubtedly to the granulites, present, like the granitic gneisses occurring in the granulite-complex and interbedded with mica-schists, the character of eruptive masses. They contain included fragments, and impregnate these as well as their immediate neighbourhood. The supposition formulated by Prof. Lehmann at the close of his researches in this district is thus confirmed—namely, that the Saxon granulite is a granite *massif*, which has been influenced in structure and composition by dynamic metamorphism.

This confirmation of his work induces the author to explain why he cannot accept the views advocated by E. Reyer in his newly-published work on "Theoretical Geology." Reyer holds the Saxon granulite-*massif* for "a mass of eruptive granite (*Massenerguss*), mantled over by 'tuffogenic' sediments (granulite), through which granite dykes are extruded from the central mass; while granite sheets (*Flankenergüsse*) are intercalated between its beds." Reyer might have gathered from the author's work that the Saxon granulites are, in the main, by no means highly metamorphosed: on the contrary, they deviate very little, in part not at all, from the original structure of eruptive granite rocks.

But apart from this, and without dwelling on the fact that we know absolutely nothing of the rocks underlying the Saxon granulites, the supposition that the alternation of mica-schists with granulite or granitic gneisses has been produced by an accumulation of successive lateral eruptions (*Flankenergüsse*) and precipitated sediments, cannot hold good.

The theoretical considerations of Reyer, the utility of which is gladly recognized by the author, and which in many cases can be supported by direct observation, must not be allowed to prejudice our judgment. The actual facts must first be established, and in so doing we do not encounter the streaky and platy structures which characterize the direction of movement in magmas. We see, in truth, something quite different. The "bedded" granite presents no zones of consolidation that follow closely the surrounding slates; we see rather an extraordinarily uniform mass of granite at first traversing, in a dyke-like manner, the slates, but afterwards insinuating itself between them, in both cases enclosing fragments of the traversed rock. Where the granite was intruded as a dyke these fragments lie without order, but where it forms a sheet the flat pieces are, almost without exception, arranged parallel to the walls of the dyke. We are accustomed to regard granite, occurring as a dyke, as younger than the rock in which was formed the crack along which the molten rock ascended, without wishing to deny that it has existed, from the very beginning, deeply hidden in the bowels of the earth, and is therefore, in reality, older than the slates it traverses. But it has become customary to observe the convention; indeed, it is necessary to do so if we do not wish to be involved in universal chaos.

For the "bedded" granite it is no simple matter to prove that it is younger than its hanging wall. Attentive examination shows that the apparently conformable boundary has no such very conformable course; that, further, the apparently sedimentary beds are sometimes distinctly detached, and turn out to be loose masses; finally, a whole series of detailed phenomena show that wherever there have been dislocations, the granite has followed the opening and has impregnated the slates. How far such an impregnation can be assumed to have taken place is a matter for personal experience.

In the granite dykes the inclusions and the boundary surfaces of the slates present exactly the same phenomena; only in this case the fragments do not all present a parallel arrangement.

One would be driven to deny the possibility of strata or slate-masses being split parallel to their stratification or their bedding, if we were to deny that the "bedded" granites do not as much constitute a case of intrusion along cracks as do the obliquely-running granite dykes. Why should there not be, among such a number of granite dykes that run unconformably, some that have been formed by the in-filling of cracks (of seldom more than 400 metres width) that follow the divisional planes of stratification or cleavage? It is not to be supposed that these were cavities, the wide sweeping arches of which were supported by the rigidity of the lateral rock-masses: as fast as the slates were separated the granite forced in its way, and filled up the crack as soon as it was developed.

This separation along parallel divisional planes and intimate impregnation with eruptive material, which can be followed in the minutest details with the greatest clearness, arouse the question as to whether the same phenomena have assumed greater dimensions—dimensions that would still be trivial in comparison with the masses erupted. The author has described several exposures in the Saxon granulite district that render any other interpretation impossible.

Kjerulf, Michel-Lévy, and others have described very similar relations among eruptive masses. Michel Lévy has quite recently given expression to his opinions in a "Note sur l'origine des terrains cristallins primitifs," and in a "Note sur les roches éruptives et cristallines des montagnes du Lyonnais." His statement to the effect that the author and a portion of the German school assume a development of heat by the plication of the earth's crust is, so far as the author is concerned, incorrect. On the contrary, he has shown that a conversion of motion into heat has left no visible traces. He is quite at one with the French investigator as to the origin of the heat in the earth's crust.

The chief requisite in the discussion of the crystalline schists, is never to leave the solid ground of facts, and to pay particular attention to the collecting of these. If the statements of some authors are examined, it must awake astonishment to see with what positiveness statements are made, which, although of the greatest importance for the proper judgment of the genesis of rocks, do not correspond in the least to the facts, and by a little attention might easily have been avoided. The author has already had occasion to disprove the non-occurrence of fragments of clay-slate in the phyllite-gneiss of Goldkronach in the Fichtelgebirge, as maintained by Gümbel. A few hours' search sufficed to collect the clearest examples of abruptly fractured and injected enclosures From this occurrence Gümbel drew widely generalized conclusions. Whether it is enclosed fragments or concretions that are contained by a rock, is assuredly of the greatest importance for its proper explication.

The author then refers to the Ober-Mittweida conglomerate. Roth has described the pebble-like fragments enclosed in this rock, as concretions. The author cannot agree with him in this determination, and gives his reasons, which are mainly based on petrographical considerations, why he does not do so.

It will hardly be denied that dynamic processes involved in mountain-building, be the latter referred to whatever cause one will, have not only not been without influence on the structure of the rocks already in existence, but have also considerably influenced the distribution and the intrusion of the eruptive masses. The observations which the author has published on the gabbros of the Saxon granulite district, on banded granulites, on the conglomerates of the Saxon Erzgebirge that have been metamorphosed to mica-schists, and on Bavarian "Pfahlschiefer," have not been refuted, although their re-examination would not have been attended by any especial difficulties.

The author then concludes by expressing the wish, that, in the further study of the crystalline schists, metamorphosed sediments, which can, it is true, be altered to true mica-schists, but never to true gneisses of uniform structure, should be kept distinct from those mica-schists of which we do not know the origin, and from the true granitic gneisses. If this be not done, the false conclusion is inevitable that sediments pass through mica-schists into gneisses. Metamorphosed, gneiss-like sediments, in which allothigenic minerals like feldspar are associated with authigenic quartz and mica, and which are, to the author's mind incorrectly, often designated gneiss, should be described as gneissose greywacke, or as gneiss-greywacke. Some such divisional line must be drawn if we are to obtain any enlightenment on the structure and origin of districts composed of crystalline schists and massive rocks. Dubious schists should be represented by a neutral colour, and not lumped in with the gneisses. The designation gneiss is meaningless as long as the most diversified crystalline and semi-crystalline schists are included under it.

Gneisses, to the author's mind, are granites possessing a parallel structure, which is partly original, partly the result of a more or less intense pressure during, or subsequent to, consolidation. Whatever else may be said, gneisses and granites are things that belong to the same category, and the author cannot reconcile himself to their separation, with respect to origin.

It cannot be natural to arrange in separate petrological categories eruptive granites and non-eruptive granites (gneisses), eruptive diorites and non-eruptive diorites, eruptive gabbros and non-eruptive gabbros, and to treat them from distinct points of view. Plutonic rocks do not differ from the similarly composed eruptive rocks, because they have not left the place in which they were formed.

How the origin of the plutonic rocks is to be conceived—whether as the primordial terrestrial crust, or as produced by the melting down or *diagenesis* of sediments, the latter supposition involving logically a similar origin for a part, at least, of the eruptive rocks—are questions of which the answers are at present less pressing, and which, with certitude, we shall probably never solve.

THE STRATIGRAPHICAL SUCCESSION OF THE CAMBRIAN FAUNAS IN NORTH AMERICA.[1]

A REVIEW of the opinion of American geologists on the succession of the Cambrian faunas shows that all have followed the scheme published by Sir William Logan in 1855 (Geological Survey, Newfoundland), in placing the Paradoxides fauna at the base, and then in succession the Olenellus and Dicellocephalus or Olenus faunas.

The discovery of *Olenellus Kjerulfi* beneath the Paradoxides zone in Sweden led me to re-examine the section of Cambrian rocks in New York, and finally to go to Newfoundland. After long search, I found a complete unbroken section on Manuel's Brook, Conception Bay, that showed the following conformable series:—

		Feet.
a.	Archæan Gneisses.	
1.	Conglomerate resting unconformably upon *a* ...	35
2.	Sandstone, shale, and impure limestone with *Olenellus Bröggeri*,[1] and sixteen species of the Olenellus fauna	25
3.	Greenish argillaceous shale	40
4.	Red ,, ,, ...	4
5.	Limestone	2
6.	Greenish argillaceous shales with an abundant Paradoxides fauna at summit	270
7.	Dark argillaceous shales, *Paradoxides*, *Microdiscus punctatus*, *Agraulus*, *Conocoryphe*, &c., &c., near base	295
8.	Alternating bands of shale and sandstone, with *Orthis* in great abundance	400
	Total	1071

Dip of strata, 12° to 15° N. All strata unaltered and undisturbed.

The above section proves that in North America, as in Sweden, the Olenellus fauna is beneath the Paradoxides fauna. This changes the American scheme of classification of the Cambrian system, and places it in harmony with that of Europe. The Olenellus fauna in America includes 42 genera and 112 species, and I now recall 4 genera and 20 species from Europe not known in America, which give a fauna of 46 genera and 132 species beneath the Paradoxides zone.

The following table exhibits the succession of the terranes as now known in America:—

TABLE I.—*Lower Silurian* (*Ordovician*) *System.*

	Subdivision.	Terranes.	Faunas.
Cambrian System.	Upper Cambrian	Potsdam, Knox, Tonto, Belle Isle, &c.	Dicellocephalus or Olenus.
	Middle Cambrian	St. John, Avalan, Braintree.	Paradoxides.
	Lower Cambrian	Georgia, Prospect, Terra Nova.	Olenellus.

A comparison of typical sections of the Cambrian system gives the following:—

TABLE II.

Sweden.	Wales.	Newfoundland.	New York.	Rocky Mountains.
Olenus zone.	Olenus zone.	Olenus zone.	Olenus zone.	Olenus zone.
Paradoxides zone.	Paradoxides zone.	Paradoxides zone.	Represented by other genera than Paradoxides.	Represented by other genera than Paradoxides.
Olenellus zone.	Unknown.[2]	Olenellus zone.	Olenellus zone.	Olenellus zone

It affords me pleasure to recognize the work of the Swedish geologists, and to fully coincide with their results, and thus firmly establish on the two continents the true order of succession of the oldest known Palæozoic fauna.

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[1] Abstract of remarks made by Chas. B. Walcott, of the United States Geological Survey, before the meeting of the International Geological Congress in London, in the course of discussion on the Cambrian System, on September 18, 1888.

[1] Name proposed for new species of Olenellus.

[2] After this paper was read, Prof. Lapworth showed me specimens of Olenellus like *O. Bröggeri*, from Shropshire.